PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Determinants of breastfeeding indicators among children less than 24 months of age in Tanzania: a secondary analysis of 2010 Tanzania Demographic and Health Survey
AUTHORS	Victor, Rose; Baines, Surinder; Agho, Kingsley; Dibley, Michael

VERSION 1 - REVIEW

REVIEWER	Mselenge Mdegela. Clinical Lecturer Insti Maternal and Newborn Health Unit, Liverpool School of Tropical Medicine. UK. Competing Interests: None.
REVIEW RETURNED	10-Jul-2012

THE STUDY	As well stated in the study, the design cannot guarantee accurate information on EBF as it was based on a 24 hours recall period. A longer period of observation or re sampling would provide a more accurate information. On Page 7 in the methodology section, the term 'size of the child' may not be familiar, a more familiar term would be better understood. C/S delivery as a very significant risk of delayed initiation of BF within one hour of birth to appear in the abstract's results section.
RESULTS & CONCLUSIONS	Most results and the resulting interpretations are credible. In my opinion few issues need clarifications though: 1. On Table 3 it is clear that the age group 25 - 34 years has a higher rate of early initiation on BF as compared to those younger or older but the authors insist that it is only the young (and mostly first time mothers) who have a low rate of early initiation of BF. This appears in the abstract and conclusion and I think it is not right. 2. On Pg 11 The rate of early initiation of BF was 52.1% and not 52.0 as reported. 3. Pg 15 about the influence of C/S on delaying initiation of BF; apart from the effects of anesthesia, some baby unfriendly post operative care practices may be responsible (from the same reference 39) 4.Partners education is similarly associated with a significant higher rate of early initiation of BF (Table 3). This has not featured in the results and discussion. 5. From reference 34, there is lack of enough documentation of how

	BFHI was implemented and monitored in the 4 regions mention, to suggest it as a cause for the observed geographical/zonal differences. May need rephrasing the explanation. 6. From Table 3 two parameters which may sound conflicting may need some explanation. The maternal working status (past 12 months) which is associated with lower rate of early initiation of BF and a higher maternal education (which has a sharp opposite association). Does this mean that most educated women (secondary and above) were in the category of Non-working? could there be other possible explanation(s) for this?
GENERAL COMMENTS	Congratulations for a hard and prudent piece of work!

REVIEWER	Dr Sia E. Msuya Clinical Lecturer Liverpool School of Tropical Medicine, L3 5QA United Kingdom I Declare I have no competing interest.
REVIEW RETURNED	21-Aug-2012

THE STUDY	Minor change is required in key messages bullet point # 3. It is employed women and not unemployed who were less likely to initiate breastfeeding within the first hour and less likely to exclusive breastfed in the univariate analysis.
GENERAL COMMENTS	**General:** The study is timely and addressing a major public health problem in developing countries including Tanzania. The manuscript is well written and data appropriately analyzed. There are minor questions the authors need to address before the manuscript is accepted for publication. **Key messages** • The third bullet point: The authors wrote that unemployment was one of the main determinants for sub-optimal breastfeeding practices in Tanzania. However when you look at Tables 3 & 4 it shows it is employed women who are less likely to initiate early BF compared to non-working women and are less likely to exclusively breastfed their infants (at bivariate analysis). This needs to be changed **Abstract**

	• Same mistake of employment as in the key messages. 'Multivariate analysis revealed the risk of delayed initiation… was significantly higher among….' Should be ..'young mothers aged < 24 years, uneducated, employed mothers and those from rural areas who ……' **Methods** • In the data analysis section, on page 8, first paragraph, the authors wrote the analysis for exclusive breast feeding and predominant breastfeeding was based on 744 infants aged from 0-5 months, however in Table 1 & 2 the number of infants aged 0-5 months is 837. Is there an explanation or a typo mistake? **Discussion** • Page 13, second paragraph, what is IYCF? • Page 16, last paragraph of the discussion has contradictory remarks. 'Infants of mothers with no formal education were significantly less likely to be exclusively breastfed than those mothers who had secondary education or higher levels of education'. This is contradictory to the results in Table 3 where mother without formal education had slightly higher prevalence of EBF (50.6%) compared to mothers with secondary education (47.8%). On the other hand the sentence is true for early initiation of breast feeding. Can the authors change this? **Tables** • Table 4: • In sub-section '*non-exclusive breast feeding'* the authors gave adjusted odds ratio with 95% CI for child age in months (< 6 months). What is the comparison group or which group are they comparing to? Exclusive breastfeeding is calculated for infants less than 6 months. It is thus not clear; can the authors either remove this or give an explanation?

REVIEWER	Anne Kjersti Daltveit professor, PhD University of Bergen Norway I have no conflicts of interests.
REVIEW RETURNED	06-Sep-2012

GENERAL COMMENTS	Abstract: The terms used are a bit confusing. For example is it referred to “non-exclusive breastfeeding” and “exclusive breast feeding indicators” – is this the same? Furthermore, “delayed initiation of breastfeeding” and “delayed initiation of breatfeeding within one hour after birth”. I suggest that the details about the indicator (time frame) should be presented at the first occurrence of the term. Also, both “exclusive breastfeeding” and “non-exclusive breastfeeding” are used - why use different terms? Page 5, lines 19-24. For persons not familiar with the indicators it would be valuable information to say how many indicators there are altogether, and that the 3 selected for further analysis are among these. Page 7, line 7 refers to “early initiation of BF indicators” – where is this acronym defined, and is this a necessary acronym? Page 10, Table 2: What is the meaning of splitting “early initiation of breastfeeding” into age intervals? A minor detail: “Continued breastfeeding at 1 year” and “Continued breastfeeding at 2 years” are separate indicators in the WHO list on page 6, but in Table 2 they are not separated with a line like the other indicators. Pages 11 and 12. Why do you use both the term “early initiation of breast feeding” (example page 11, line 21) and the term “delayed initiation of breastfeeding” (example page 12, line 6) ? To me, different terms add to the confusion. Page12, line 8. Why do you explain the term “non-EBF” here while it is used already at page 8, line 5?

	General comments on results and discussion: Three indicators (early initiation of breastfeeding < 1 hour after birth, exclusive breastfeeding <6 months and predominant breastfeeding <6 months) are analysed in detail. Table 3 shows that the early initiation indicator is associated with most characteristics, while the two other indicators are less associated with the characteristics. Why is early initiation then important, if the other breastfeeding indicators are more or less independent of the same factors that determine early initiation? To me, an analysis of the association between early initiation and the two other indicators would be interesting. Does early initiation of breastfeeding predict exclusive or predominant breastfeeding? Most of the discussion is related to the early initiation indicator. The importance of this indicator should be justified before conclusions are drawn.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
As well stated in the study, the design cannot guarantee accurate information on EBF as it was based on a 24 hours recall period. A longer period of observation or re sampling would provide more accurate information.
Comment 1:
On Page 7 in the methodology section, the term 'size of the child' may not be familiar, a more familiar term would be better understood.
Response 1:
Thank you for the comment; we have adjusted this term and now reads “Perceived size of the baby” (page 7 line 13).

Comment 2:
C/S delivery as a very significant risk of delayed initiation of BF within one hour of birth to appear in the abstract's results section.
Response 2:
Thank you for the comment. We have included caesarean delivery as a risk factor for delay initiation of breastfeeding within 1 hour after birth in the abstract (page 3 line 19).

Comment 3:
Most results and the resulting interpretations are credible. In my opinion few issues need clarifications though: On Table 3 it is clear that the age group 25 - 34 years has a higher rate of early initiation on BF as compared to those younger or older but the authors insist that it is only the young (and mostly first time mothers) who have a low rate of early initiation of BF. This appears in the abstract and conclusion and I think it is not right.
Response 3:
Thank you for the observation, we agree with you that mothers aged 25-34 years had higher rate of early initiation of breastfeeding than younger (15-24 years) and older mothers (35-49 years) in the Univariate analysis. However, after adjusting for other potential confounders such as level of education, employment status, mode of delivery, area of residence etc.... mothers aged 25-34 years and those aged more than 35 years had significantly lower risk of delay initiation of breastfeeding within 1 hour after birth compared to younger mothers aged < 25 years. We further explore this

association with parity and found that younger mother were first time mothers hence we drew our conclusion as stated in our manuscript.

Comment 4:
On Pg 11 The rate of early initiation of BF was 52.1% and not 52.0 as reported.
Response 4:
Thank you, we have adjusted this figure in our revised manuscript (page 11 line 17)
Comment 5:
Pg 15 about the influence of C/S on delaying initiation of BF; apart from the effects of anesthesia, some baby unfriendly post operative care practices may be responsible (from the same reference 39)

Response 5:
Thank you for the input, we have incorporated this information in our revised manuscript (page 15 line 28)

Comment 6:
Partner's education is similarly associated with a significant higher rate of early initiation of BF (Table 3). This has not featured in the results and discussion.
Response 6:
Partner's education was associated with significant higher rate of early initiation of breastfeeding in the Univariate analysis but not in the multivariate analysis. We have presented this finding in the results section in our revised manuscript (page 11 line 20-21) but the readers and policy makers will be interested on the multivariate analysis after adjusting with other potential confounders.

Comment 7:
From reference 34, there is lack of enough documentation of how BFHI was implemented and monitored in the 4 regions mention, to suggest it as a cause for the observed geographical/zonal differences. May need rephrasing the explanation.
Response 7:
Thank you for the comment, we have rephrased the explanation in our revised manuscript (page 14, line 23-26).

Comment 8:
From Table 3 two parameters which may sound conflicting may need some explanation. The maternal working status (past 12 months) which is associated with lower rate of early initiation of BF and a higher maternal education (which has a sharp opposite association). Does this mean that most educated women (secondary and above) were in the category of Non-working? could there be other possible explanation(s) for this?
Response 8:
Yes, we concurred with your comment and the result in Table 5a for reviewer's comments indicated a correlation between non-working status and secondary or higher level of education, with 22.4% of mothers who initiated early initiation of breastfeeding within 1 hour after birth had secondary or higher education level and were not working compared to those mothers working with secondary or higher education level (12.9%). In this study a working mother was defined as woman who is working away from home. When we explored the correlation between work and area of residence (Table 5b for reviewer's comments), we found that most of working mothers resided in the rural areas (68.6%) than in urban areas (12.5%) and they had higher risk of delayed initiation of breastfeeding as discussed in our manuscript (page 15, line 1-6).

Reviewer 2:
Dr Sia E. Msuya
Clinical Lecturer
Liverpool School of Tropical Medicine, L3 5QA
United Kingdom

I Declare I have no competing interest.

Minor change is required in key messages bullet point # 3. It is employed women and not unemployed who were less likely to initiate breastfeeding within the first hour and less likely to exclusive breastfed in the univariate analysis.

General:
The study is timely and addressing a major public health problem in developing countries including Tanzania. The manuscript is well written and data appropriately analyzed. There are minor questions the authors need to address before the manuscript is accepted for publication.

Comment 1:
Key messages
• The third bullet point: The authors wrote that unemployment was one of the main determinants for sub-optimal breastfeeding practices in Tanzania. However when you look at Tables 3 & 4 it shows it is employed women who are less likely to initiate early BF compared to non-working women and are less likely to exclusively breastfed their infants (at bivariate analysis). This needs to be changed
Response 1:
Thank you for your comment. We agree with you and we have corrected this sentence in our revised manuscript (Page 2, line 15).

Comment 2:
Abstract
• Same mistake of employment as in the key messages. 'Multivariate analysis revealed the risk of delayed initiation… was significantly higher among….' Should be ..'young mothers aged < 24 years, uneducated, employed mothers and those from rural areas who ……'
Response 2:
Thank you for the comment and we agree with you. We have modified this sentence in our revised manuscript (Page 3, line 18).

Comment 3:
Methods
• In the data analysis section, on page 8, first paragraph, the authors wrote the analysis for exclusive breast feeding and predominant breastfeeding was based on 744 infants aged from 0-5 months, however in Table 1 & 2 the number of infants aged 0-5 months is 837. Is there an explanation or a typo mistake?
Response 3:
Thank you for your comment. It was a typo mistake and we have corrected this figure in our revised manuscript. The sample size for infants aged 0-5 months was 837 (page 8 line 2).

Comment 4:
Discussion
• Page 13, second paragraph, what is IYCF?
Response 4:
-The term IYCF stands for infant and young child feeding. We have defined this term in our revised manuscript (Page 13, line 17).

Comment 5:
Page 16, last paragraph of the discussion has contradictory remarks. 'Infants of mothers with no formal education were significantly less likely to be exclusively breastfed than those mothers who had secondary education or higher levels of education'. This is contradictory to the results in Table 3 where mother without formal education had slightly higher prevalence of EBF (50.6%) compared to mothers with secondary education (47.8%). On the other hand the sentence is true for early initiation of breast feeding. Can the authors change this?
Response 5:
Thank you for the comment, we agree with you that mothers with no formal education had slightly higher prevalence of EBF compared to mothers with secondary or higher levels of education in the univariate analysis. However, this finding was not statistically significant different in either univariate or multivariate analysis. Therefore, we have removed this information from the discussion of our revised manuscript.

Comment 5:
Tables

• Table 4:
• In sub-section ‘non-exclusive breast feeding’ the authors gave adjusted odds ratio with 95% CI for child age in months (< 6 months). What is the comparison group or which group are they comparing to? Exclusive breastfeeding is calculated for infants less than 6 months. It is thus not clear; can the authors either remove this or give an explanation?
Response 5:
Thank you for the comment; child age was used as a continuous variable and can’t be compared with any category. The aim of doing this was to test whether EBF increases as child age increases or if EBF decreases as child age increases.

Reviewer 3
Anne Kjersti Daltveit
professor, PhD
University of Bergen
Norway

I have no conflicts of interests.

Comment 1:
Abstract: The terms used are a bit confusing. For example is it referred to “non-exclusive breastfeeding” and “exclusive breast feeding indicators” – is this the same? Furthermore, “delayed initiation of breastfeeding” and “delayed initiation of breastfeeding within one hour after birth”. I suggest that the details about the indicator (time frame) should be presented at the first occurrence of the term. Also, both “exclusive breastfeeding” and “non-exclusive breastfeeding” are used - why use different terms?
Response 1:
Thank you for the good comment; we have defined these terms in our revised manuscript (page 8 line 31-32 and page 9, line 1-2). However, “non-exclusive breastfeeding and exclusive breastfeeding indicators are statistically not the same. For policy makers and researchers view point, it would be wise to target those mothers not practicing exclusive breastfeeding (those mothers doing inappropriate feeding practice) than mothers who practice exclusive breastfeeding (those doing appropriate feeding practice).

Comment 2:
Page 5, lines 19-24. For persons not familiar with the indicators it would be valuable information to say how many indicators there are altogether, and that the 3 selected for further analysis are among these.
Response 2:
Thank you, we have added this information in our revised manuscript (page 6 line 12 and page 7, line 19-28).

Comment 3:
Page 7, line 7 refers to “early initiation of BF indicators” – where is this acronym defined, and is this a necessary acronym?
Response 3:
Thank you for the comment. We have removed the word BF and replace it with breastfeeding (Page 7 line 1).

Comment 4:
Page 10, Table 2: What is the meaning of splitting “early initiation of breastfeeding” into age intervals?
Response 4:
The reason of splitting indicator for “early initiation of breastfeeding” into interval was based on the research aim and on breastfeeding indicators which were based on WHO 2008 recommendation. Our objective states “examine prevalence breastfeeding indicators in Tanzania and determine factors associated by key breastfeeding indicators.

Comment 5:

A minor detail: “Continued breastfeeding at 1 year” and “Continued breastfeeding at 2 years” are separate indicators in the WHO list on page 6, but in Table 2 they are not separated with a line like the other indicators.
Response 5:
We agree with your comment, we have separated with a line the indicators for “Continued breastfeeding at 1 year” and Continued breastfeeding at 2 years in the revised manuscript (Table 2 page 10).

Comment 6:
Pages 11 and 12. Why do you use both the term “early initiation of breast feeding” (example page 11, line 21) and the term “delayed initiation of breastfeeding” (example page 12, line 6)? To me, different terms add to the confusion.
Response 6:
We used the term “early initiation of breastfeeding within 1 hour after birth” in page 11 because we want to report the prevalence of “early initiation of breastfeeding within 1 hour after birth”. However, policy makers and researchers would like to target those mothers who delayed to initiate breastfeeding within 1 hour after birth. That is why our multivariate analysis was based on delayed initiation of breastfeeding within 1 hour after birth in page 12.

Comment 7:
Page12, line 8. Why do you explain the term “non-EBF” here while it is used already at page 8, line 5?
Response 7:
We have explained the term non-EBF in page 8 line 3 and removed it in page 12 in our revised manuscript.

Comment 8:
General comments on results and discussion: Three indicators (early initiation of breastfeeding < 1 hour after birth, exclusive breastfeeding <6 months and predominant breastfeeding <6 months) are analysed in detail.
Table 3 shows that the early initiation indicator is associated with most characteristics, while the two other indicators are less associated with the characteristics. Why is early initiation then important, if the other breastfeeding indicators are more or less independent of the same factors that determine early initiation? To me, an analysis of the association between early initiation and the two other indicators would be interesting. Does early initiation of breastfeeding predict exclusive or predominant breastfeeding?
Response 8:
Thank you for the comment and this idea of “Does early initiation of breastfeeding predict exclusive or predominant breastfeeding?” could be the second paper from this study but analysis on factors associated with sub-optimal infant and young child feeding practices are urgently needed for Tanzania to identify group of individuals with poor practices that need to be targeted for nutrition programs to improve their IYCF practices that will contribute to reach the child survival Millennium Development Goal of reducing infant and child mortality by the year 2015.

Comment 9:
Most of the discussion is related to the early initiation indicator. The importance of this indicator should be justified before conclusions are drawn.
Response 9:
We agree with you but this could be justified once we have explored the association as suggested on the comment above.

VERSION 2 – REVIEW

REVIEWER	Anne Kjersti Daltveit professor, PhD

	University of Bergen Norway I have no conflicts of interests.
REVIEW RETURNED	01-Nov-2012

GENERAL COMMENTS	The authors have addressed most comments. Still, the importance of the early breastfeeding indicator seems unclear to me, but the authors argue that this could be addressed in a next paper. I have no more comments.